of the death of 3,000 Indians from small-pox in the Saskachewan district. It is then added "that martens that are not killed, and Indians that die, mean reduced dividends to the Hudson's Bay shareholders and traders."

Having lived a good many years in the Hudson's Bay Territory, perhaps you will permit me to mention a curious circumstance which I noticed, in illustration that martens may abound yet comparatively very few be killed.

In all parts of the fur country east of the Rocky Mountains, where there is timber, hares (*Lepus americanus*), or "rabbits," as they are commonly, but wrongly, called, are found in greater or less numbers, and they congregate in certain favourite localities. The Indian pitches his tent near one of these places, and by setting snares (which his wife and children attend to), easily supplies himself and family with food, whilst the skins of the hares are worked up into most comfortable blankets.

The hunter all the while is trapping the marten and other fur-bearing animals that assemble to prey upon the poor rabbits, and is thus enabled to secure without much labour a large and valuable stock of furs, chiefly martens.

The hares are, however, liable to a very fatal epidemic,[*] which usually attacks them when they have become very numerous, and they gradually die off, so that in two or three years there is scarcely one to be seen. This scarcity continues for a couple of winters or so, after which the hares again begin to increase, so that at periods of eight or ten years they are at their maximum.

During this dearth of hares, the Indian has to go to a fishery, or is obliged to travel about in search of buffalo, deer, or other game as a means of support, and has little time for trapping the marten; and if he had the time, he would still be under great disadvantage, for the marten, lynx, and fisher have also to scatter themselves all over the country to pick up a precarious living on lemmings, partridges, and other odds and ends, instead of feasting in luxury and ease, as they do, on the hares when abundant. Thus, when hares are numerous, many marten skins are obtained, when hares are few marten skins are also few, not necessarily because martens are scarce, but that they are difficult to get.

The death of even 3,000 prairie Indians in one season, however injurious it might be to the trade of the Hudson's Bay Company in other kinds of furs, would not particularly affect the number of marten skins obtained.

I may here record a striking instance of the efficacy of vaccination as a preventative of small-pox. Nearly forty years ago this dreadful disease spread like a scourge from the Missouri river all over the prairies, being carried by bands of horse-stealers from one tribe to another; for these amiable "children of nature" no sooner heard of any of their neighbours being attacked by the terrible disorder, than parties went immediately to rob the sufferers of their most valuable property. They got the horses, but they also caught the disease, and many hundreds died. The Crees, a tribe of many thousands, having nearly all been vaccinated by the Hudson's Bay Company's officer in charge of the district, escaped with the loss of only two of their number.

JOHN RAE

Ripples and Waves

THE article by Sir William Thomson upon Ripples and Waves in the November part of NATURE, which has just reached me, reminds me of a little capillary wave, the examination of which used to be a source of amusement to me some years ago; and as I have never seen any description of it, my observations may not be without interest to some of your readers.

I had long noticed this little wave, winding about, like a hair upon the surface, amongst the eddies which formed in a deep river below a considerable fall, which I used to frequent; but I first got an insight into its nature in a very different situation. I was in a canoe in a sheltered bay, with just enough wind overhead, without any ripple on the water, to make my canoe drift broadside on at the rate of, perhaps, half-a-mile an hour, when I saw my little wave formed about three feet in advance of the canoe. Being in the neighbourhood of a marsh the water was very impure, and the behaviour of the little particles floating in it attracted my attention. Any objects reaching to the depth of from an eighth to a quarter of an inch below the surface passed on to the canoe unaffected by it; but smaller particles were suddenly agitated on passing the wave, and after getting a few inches within it, they were arrested at distances varying with their size, the larger ones penetrating farther than the smaller ones. If the wind died away the wave was maintained at a greater distance from the canoe, and it was still perceptible at a distance of fully eight or nine feet from it, after which it became fragmentary and disappeared. If the pace of the canoe increased, the wave came nearer to it, and the particles, which had been brought to rest at various intervals according to their sizes, were driven up together, forming at last a sort of scum in advance of the canoe. If the wind increased suddenly, the wave disappeared, and the slightest ripple on the surface obliterated it at once; but if the wind freshened very gradually, the wave approached nearer and nearer, becoming at the same time more strongly defined, until it came to within about nine inches from the canoe, and was maintained there under its lee, even after there was breeze enough to make a considerable ripple outside. If pressed beyond that, ripples of quite another character would form just in advance of the wave, and it would break up, and the canoe would pass over the scum which had collected within it.

With this clue as to its nature, I frequently examined the wave in the situations where I had first seen it. Wherever there was any impediment to the stream, as a tree stretching out into it from the bank, there was the little wave ahead of it, at distances from the impediment varying with the force of the current. In the spring, when the water was high, a good deal of foam would be brought down from the falls above, and would collect against these obstructions, but always leaving an inch or two of clear water within the wave. Upon clearing away the foam the wave would soon again be formed, and the next patch of foam which came down would experience a little jerk, as it passed the wave, and penetrate a few inches within it, when it would be arrested, and there would start out from underneath it little particles of sawdust, or other substances, which had been entangled in it, and would range themselves beyond it, in the order of their sizes. Presently more foam would come down, pushing on what had arrived before, till soon there would be an accumulation of it, as at first.

Where the wave was found winding about amongst the eddies there was no solid obstacle, but only one stream meeting another, and it was not at first sight easy to distinguish which was the front and which the back of the wave. The accumulation of scum, however, on one side showed this, and much more so the behaviour of the wave itself, according to the side from which you approached it. If you came down upon it with the stream, with your canoe broadside on, no effect was produced on the wave; but if you passed over it, it was almost immediately reformed on the other side. But if you approached it from the other side, you pushed it on before you; and by careful handling I have often succeeded in detaching a portion of the wave, and carrying it on before me for ten or fifteen yards; whilst after awhile another would be formed in the same place. Sometimes, where the water boiled up from below, there would be an irregular circular patch, surrounded by one of these waves, which you might drive up till the two sides met; or if you approached it stern on, you would cut the circular patch into two, in which case each would run up rapidly to their centre into a little conical jet, and if your pace was at all rapid there would be a drop projected upwards from it.

The wave is so minute that it was not easy to come to any conclusion as to its shape and size; but from the distorted reflection of an object held above it I satisfied myself that under ordinary circumstances it could not be more than one-twentieth of an inch high, the distortion not extending beyond half-an-inch on each side of the sharp cusp, and that it was convex towards the stream, with a very slight trace of concavity on the side of the obstacle generating it. It seemed as if the wave itself was a little elevated above the surface, and that it sloped back very slowly towards the obstacle. This is in accordance with the description above given of a narrowing circular patch running up to a jet; for, although the motion in that case was too rapid to permit of any precise observation, just before it closed in the patch had the appearance of a little table land elevated above the general surface. Upon one occasion, when a boom had been stretched across the river, running at the time fully five or six miles an hour, the wave was only about nine inches from the boom, against which a dense scum was collected, but still with about an inch of clear water between it and the wave. The wave in this case must have been fully an eighth of an inch high, and on its farther side were a succession of ripples, very much exceeding the capillary wave in height and amplitude, and differing

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[*] It is quite as fatal in its effects as the grouse disease, and the causes are little known The hares are found sitting in their forms dead. The Indians say they can tell when the disease is about to commence by a peculiar growth found in the abdomen.

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from it in not being cusped, though otherwise imitating its general form.

It would appear, therefore, as if a wedge-shaped film of water were pushed ahead of the canoe, or other obstacle, the lower surface of which must, from the arrangement of the particles arrested, have been of rapidly-increasing curvature. Two difficulties, however, present themselves to this explanation—it is difficult to see how the film could have extended to the wave itself, as no particles, however small, appeared to be arrested within an inch or two of it; and my recollection is that upon the occasion of my first examining the wave driven before my canoe, light objects merely resting upon the water, like thistle down, seemed to be not at all affected by it, but to pass on towards the canoe unimpeded. Such objects, however, are so easily affected by the wind, or even the resistance of the air, that it was not easy to verify the observation.

Some other facts may be mentioned. The depth of the obstruction in the water seemed to have no sensible effect on the wave formed. Whether it was a log a foot through, or an inch board floating on the water, or whether it was the middle of the canoe drawing five or six inches, or the bow and stern barely touching the surface, the effect seemed almost the same. I have often, indeed generally, failed in my attempts to generate a wave with a canoe, and although upon the occasion when I first saw it so formed, I could trace it at fully eight feet from the canoe, I never found such a wave naturally formed at anything like that distance. The explanation appears to be that it requires very even and steady action to generate the wave; but that when once established it can be maintained under circumstances in which it would not be otherwise produced. As I stated before, if you approach it in one direction, you may take a canoe over it and it emerges on the other side unimpaired; the irregular currents of an eddy have no effect upon it except to give it an undulating motion, and I have seen it maintaining its place amongst the standing waves of a rapid when they have been several inches high. I have even raised considerable swells by rocking a canoe close to it, and it rides over them without disturbance; but the slightest ripple caused by the wind makes it disappear in a moment; and if spirits of turpentine be dropped on the water a little above it, the whole wave is instantly obliterated to a distance apparently far beyond that to which the oily film extends.

JOHN LANGTON

Ottawa, Canada, Dec. 28, 1871

The Rigidity of the Earth

ALTHOUGH, as he truly says, Sir W. Thomson's arguments for the rigidity of the earth have never been attacked, yet they have undoubtedly been too long ignored; and it is gratifying to see them asserted by their author in NATURE. Allow me, however, to remark on one sentence near the end of his quotation from the "Natural Philosophy," where Mr. Hopkins's observation is given, that the distribution of fluid matter within the earth is "probably quite local." Unless I am mistaken, Mr. Hopkins's opinion was, that its distribution is, as one might say, fortuitous. But, as I have elsewhere observed, the trains of volcanoes which accompany many of the great lines of elevation for enormous distances render the motion of such local distribution of fluid matter highly improbable, unless it be admitted that its presence is due to mountain elevations as a cause. I have suggested that this fluidity may arise from a diminished pressure beneath mountain ranges, owing to their mass being partly supported by the lateral thrust which has upraised them—a supposition which Mr. Scrope had already applied to account for an increased fluidity in the heated rock underlying a volcanic vent, when from any cause the pressure became less.

If any of your correspondents can propose another explanation of this remarkable coincidence compatible with the supposition of a rigid globe, it would be interesting to know it.

Harlton, Cambridge O. FISHER

English Rainfall

IN reply to the letter of Mr. Vernon, in NATURE of the 18th inst., permit me to say that the confusion between the two Seathwaites is *his*, not *mine*. In the article to which he refers there is not a word about either Cockley Bridge or the Valley of the Duddon. His topographical knowledge of the districts is, apparently, as inexact as his manner of reading; for he does not seem aware that "the Stye," of which he speaks, is the name, not of a *place*, but of a *rain-gauge*, in, as I said before, the immediate neighbourhood of Stockley Bridge.

J. K. L.

Circumpolar Lands

IN the last number of NATURE (Jan. 18), Mr. J. J. Murphy asks, "Can any mathematical reason be assigned why the contraction of the earth should be least in the direction of the polar direction? This would account for the rising of the land at the poles."

In the Proceedings of the Literary and Philosophical Society of Liverpool for Nov., 1857, there is a paper on a probable change in the earth's form, in which the rising of the land at the poles is inferred as a necessary result of the cooling and contraction of the earth.

The following is the substance, though not the exact words, of a portion of the paper; the precise words would not be intelligible without a diagram.

If a spheroid of equilibrium, in motion about an axis, contract uniformly in the direction of lines perpendicular to its surface, a new spheroid is produced, having a greater degree of eccentricity, because if equal portions are taken off the two diameters, the ratio of the equatorial to the polar diameter is increased. This is equivalent to a heaping up of matter around the equator in excess of what is due to the velocity of rotation, an increased pressure on the interior, in that region, must be produced, and a consequent transmission of pressure towards the poles. "A change of form is then necessary to restore equilibrium. This may not take place uniformly *per gradum*, for if there be a resistance from a rigid external crust, the force must accumulate until it exceeds the resistance, and thus frequent adjustments *per saltum* may ensue. It is probable, therefore, that the earth's form is undergoing a slow progressive change."

GEORGE HAMILTON

Queen's College, Liverpool, Jan. 21

The Kiltorkan Fossils

MR. BAILY's letter needs only a word or two from me.

I must protest against my reference to an error made by Mr. Baily being considered a "personal attack" upon him, or an "accusation" against him. Has Mr. Baily ever consulted a systematic work which did not contain corrections of the real or supposed errors of former workers? And did he consider such corrections as "personal attacks"?

On two points Mr. Baily has misunderstood or misread the plain statements of my letter:—1. I did not say that his drawing in "Explanation of Sheets 187, &c.," was made on the spot at Kiltorkan, but that it was a drawing of the fossil he had named *Sagenaria Veltheimiana*; 2. The qualifying phrase, "coal measure," was used, as it often is, as the equivalent of "carboniferous." How Mr. Baily could make it mean anything else perplexes me; seeing the Upper Carboniferous beds have no connection with the question. To have used it in the limited sense he suggests, and elaborately argues against, would have been absurd.

The remainder of Mr. Baily's letter is occupied with reference to private letters as evidence in the case. That written by Mr. Baily to Prof. Heer confirms the statement I made at the Geological Society, and repeated in your pages; but, in as far as it declares that the specimens sent to Prof. Heer from Kiltorkan were named *S. Bailyana*, it differs from the statement made by Prof. Heer at the Geological Society, who, on the evidence of these fossils, included *S. Veltheimiana* among the Kiltorkan fossils, and never mentioned *S. Bailyana!*

The reference to the other private letters is equally unhappy; for Mr. Baily is quite wrong in supposing my "accusation" was made because I could not persuade him to join me in work. My letter, if he will look at it again, bears a date some time after the "accusation" was made. And if at the same time he will read his reply, he will find that the reason he gave for declining to work with me is somewhat different from those he records in your pages. But the fact is, the letters have nothing whatever to do with my declaration, now more than ever confirmed by Mr. Baily's letter, that his giving to the Irish Lepidodendroid plant the name of a carboniferous species misled Prof. Heer. If Mr. Baily's letter indicates the "facts" contained in his paper, I can only conclude that it was the patriotism of your reporter that induced him to characterise them as "strong."

W. CARRUTHERS

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